itself in the discussion of the solar inequalities, it appeared to the author very desirable to examine whether there is any discoverable difference in the lunar inequalities for the same years. The years were accordingly thus divided:—

Large solar curves.. 1848 to 1852, 1859, 1860, 1862, 1863.
Small solar curves.. 1853 to 1858, 1861.

On discussing these, it was found that in all cases the lunar horary epoch for the inequality was sensibly the same for years of large solar curves and for years of small solar curves; but the coefficient was different. The value of the fraction

$$\frac{\text{lunar semidiurnal inequality in years of large solar curves}}{\text{lunar semidiurnal inequality in years of small solar curves}}$$

is

For declination ..... 1·35
For horizontal force ..... 1·25

The author remarks that it would seem possible to suggest two conjectural reasons for this remarkable association in the time-law of changes of solar effect and lunar effect. One is, that the moon's magnetic action is really produced by the sun's magnetic action; and a failure in the sun's magnetic power will make itself sensible, both in its direct effect on our magnets and in its indirect effect through the intermediation of the moon's excited magnetism. The other is that, assuming both actions (solar and lunar) to act on our magnets indirectly by exciting magnetic powers in the earth, which alone or principally are felt by the magnets, the earth itself may have gone through different stages of magnetic excitability, increasing or diminishing its competency to receive both the solar and the lunar action.

The epochs of lunar inequality in western declination from north and in horizontal force to magnetic north are sensibly the same; and the coefficients expressed in terms of horizontal force on the mean of all the years are sensibly the same, and equal to 0·000061. The direction of the composite disturbing force is therefore sensibly N.W. and S.E. magnetic, or (roughly) in the direction of a line from the Red Sea to the south of Hudson's Bay. It may be remarked in opposition to this that the solar diurnal action is mainly in the S.W. direction.

The luno-diurnal inequality of vertical force on the mean of all the years appears to consist of a luno-diurnal and a luno-semidiurnal term.

December 17, 1868.

Capt. RICHARDS, R.N., Vice-President, in the Chair.

The following communications were read:—

I. "On the Measurement of the Luminous Intensity of Light." By William Crookes, F.R.S. &c. Received June 27, 1868.

(Abstract.)

The measurement of the luminous intensity of a ray of light is a problem the solution of which has been repeatedly attempted, but with less satisfactory results than the endeavours to measure the other radiant forces. The problem is susceptible of two divisions, the absolute and the relative measurement of light.

A relative photometer is one in which the observer has only to ascertain the relative illuminating powers of two sources of light, one of which is kept as uniform as possible, the other being the light whose intensity is to be determined. It is therefore evident that one great thing to be aimed at is an absolutely uniform source of light. In the ordinary process of photometry the standard used is a candle, defined by Act of Parliament as a "sperm of six to the pound, burning at the rate of 120 grains per hour." This, however, is found to be very variable, and many observers have altogether condemned the employment of test-candles as light-measures.

The author has taken some pains to devise a source of light which should be at the same time fairly uniform in its results, would not vary by keeping, and would be capable of accurate imitation at any time and in any part of the world by mere description. The absence of these conditions seems to be one of the greatest objections to the sperm-candle. It would be impossible for an observer on the continent, ten or twenty years hence, from a written description of the sperm-candle now in use, to make a standard which would bring his photometric results into relation with those obtained here. Without presuming to say that he has satisfactorily solved all difficulties, the writer believes that he has advanced some distance in the right direction, and pointed out the road for further improvement.

A glass lamp is taken of about 2 ounces capacity, the aperture in the neck being 0·25 inch in diameter; another aperture at the side allows the liquid fuel to be introduced; this consists of alcohol of sp. gr. 0·805, and pure benzol boiling at 81° C., which are mixed together in the proportion of five volumes of the former and one of the latter. The wick-holder consists of a platinum tube, and the wick is made of fifty-two pieces of platinum wire, each 0·01 inch in diameter. The flame of this lamp forms a perfectly shaped cone, the extremity being sharp, and having no tendency to smoke; without flicker or movements of any kind, it burns when protected from currents of air at a uniform rate of 136 grains per hour.

There is no doubt that this flame is very much more uniform than that of the sperm-candle sold for photometric purposes. Tested against a candle, considerable variations in relative illuminating power have been observed; but on placing two of these lamps in opposition, no such variations have been detected.

The instrument devised for measuring the relative intensities of the standard and other lights is next described; it has this in common with that of Arago described in 1833, as well as with those described in 1853 by Bernard, and in 1854 by Babinet, that the phenomena of polarized light are used for effecting the desired end*. But it is believed that the present arrangement is quite new, and it certainly appears to answer the purpose in a way which leaves little to be desired. The instrument cannot be described without the aid of drawings which accompany the original paper, but its mode of action may be understood by the following description.

The standard lamp being placed on one of the supporting pillars which slide along a graduated stem, it is moved along the bar to a convenient distance, depending on the intensity of the light to be measured. The light to be compared is then fixed in a similar way on the other side of the instrument. On looking through the eyepiece two brightly luminous disks will be seen, of different colours. One of the lights must now be slid along the scale until the two disks of light, as seen in the eyepiece, are equal in tint. Equality of illumination is easily obtained; for, as the eye is observing two adjacent disks of light which pass rapidly from *red-green* to *green-red*, through a neutral point of no colour, there is no difficulty in hitting this point with great precision. Squaring the distance between the flames and the centre will give inversely their relative intensities.

The delicacy of this instrument is very great. With two lamps, each about 24 inches from the centre, it is easy to distinguish a movement of one of them to the extent of one-tenth of an inch to or fro,and by using the polarimeter an accuracy exceeding this can be attained.

The employment of a photometer of this kind enables us to compare lights of different colours with one another. So long as the observer, by the eyepiece alone, has to compare the relative intensities of two surfaces respectively illuminated by the lights under trial, it is evident that, unless they are of the same tint, it is impossible to obtain that absolute equality of illumination in the instrument which is requisite for a comparison. By the unaided eye one cannot tell which is the brighter half of a paper disk illuminated on one side with a reddish, and on the other with a yellowish light; but by using the photometer here described the problem becomes practicable. When the contrasts of colour are very strong (when, for instance, one is a bright green and the other scarlet) there is difficulty in estimating the exact point of neutrality; but this only diminishes the accuracy of the comparison, and does not render it impossible, as it would be according to other systems.

* Since writing the above, I have ascertained that M. Jamin had previously devised a photometer in which the principle adopted in the one here described is employed, although it is carried out in a different and, as I believe, a less perfect manner.—W. C., Dec. 16, 1868.